

Bryde's whale Status Review Report (ID 337)
Peer Review Report

Peer reviewers:

Robin S. Waples
National Marine Fisheries Service

Timothy J. Regan
Executive Director (retired)
U.S. Marine Mammal Commission

John A. Hilderbrand
Professor, Scripps Institution of Oceanography
Adjunct Professor, Department of Electrical and Computer Engineering
University of California San Diego

General directive:

1. Review the scientific information contained within the status review report.
2. Identify any missing literature and assess if the Status Review Team properly interpreted the research.
3. If you believe that justification is lacking or specific information was applied incorrectly in reaching specific conclusions, please specify.

Reviewer #1

On the whole I found the status review document to be clearly written and well organized. The major conclusions seem logical and well supported by the best available information. Although official cetacean taxonomy is notoriously slow, because it generally depends on availability of skeletons, I agree that available evidence suggests that the GOMx whales merit subspecies status, which would make the DPS question moot. However, given ongoing uncertainty on this issue that is not likely to be formally resolved soon, it would have been prudent for the team to have gone through the DPS exercise, just in case that becomes necessary. Given the major genetic differences from all other known groups of whales, I don't imagine there would be any question that GOMx whales are at least a DPS, although figuring out which species or subspecies they are a DPS of might be more challenging.

Given its importance for the "species" determination, I think the report should include a table showing some of the information from Table 2 in Rosel and Wilcox 2014 that documents the extent of genetic differentiation among the various forms. Also, I would have liked to see a bit more about differentiation at nuclear genes, since patterns can differ strongly from those at mtDNA. The report only focused on low levels of nuclear diversity, which is important but for different reasons. Rosel and Wilcox mentioned that most microsatellite alleles were shared across species, which can be due to homoplasy.

The threats analysis seems much more extensive than needed, given that the estimated population size is so small that small-population concerns dominated the risk assessment. However, these analyses are likely to be useful in guiding conservation and management actions in the future. Use of the four-risk-factor Viable Salmonid Populations framework developed by McElhany et al. (2000) was a nice addition. Although this framework was developed for Pacific salmon, it is quite general so should apply to all species. I believe there was a guidance memo from NMFS HQ suggesting that all future status reviews consider these four factors, so it would be useful to cite that document. Also, the recent green sea turtle status review (Seminoff et al.

2015 NOAA Technical Memorandum, NOAA-NMFS-SWFSC-539) used these same four factors but modified them a bit to better describe 'viable turtle populations' (hence VTP rather than VSP). Citing that document would help forge links among NMFS status review teams working on common themes but with a wide range of species.

In assessing current status and prospects for recovery, it is very useful to have an understanding of how the species in question reached the point at which it is being considered for ESA listing. Historical patterns of distribution and abundance, as well as anything that is known about any aspects of behavioral/ecological/life history/genetic diversity, are particularly important in this regard. I realize that a great deal of uncertainty would be associated with any such reconstruction for GOMx whales, but nevertheless it would be useful even if all that could be done was to identify several historical scenarios

that might be consistent with the currently available information. Perhaps then it would be possible to identify types of studies that could be conducted to narrow down the range of possibilities.

Minor comments:

“As the GOMx whales are more closely related to Eden's whales than to Bryde's whales (Rosel & Wilcox 2014), the absence of Eden's whales from the Atlantic means there is a substantial distributional hiatus between the GOMx whales and their nearest congener.”

I doubt the importance of this point. Based on the Rosel and Wilcox paper, the relationships of these three type of whale are not well resolved, and the GOMx whales differ from each of the named subspecies by 25 fixed differences at mtDNA. Also, all seem to be distantly-enough related to each other that current distributions have little relevance to what they might have been historically.

“Criteria for qualifying as a DPS reference “the taxon”, which assumes that there is no uncertainty in taxonomic status.”

The joint DPS policy indicates that ‘significance’ should be evaluated with reference to “the taxon to which it [the unit under consideration] belongs’. Nothing in the policy mentions an assumption of no uncertainty in taxonomic status, which is common for many types of organisms.

“They do not appear to undertake long latitudinal migrations.” Does this refer to E-W migrations (along parallels of latitude) or N-S migrations (across parallels of latitude)?

Discussion of the 50/500 rule is a bit incomplete. The 50 value is meant to be a short-term minimum to minimize adverse effects of inbreeding. It was based in part on observations of levels that could be tolerated in domesticated animal populations. The 500 value was meant to reflect an effective size for which a rough balance might be achieved between loss of diversity through drift and gain of diversity through mutation. Both values are only meant as rough guides. As noted in the report, some have recently argued that the guidance be changed to 100/1000, but no consensus has been reached on this point.

The short section on Climate Change should reference the Climate Science Strategy (Link et al. 2015) that NMFS released last year, which mandates that all management-related actions by the agency consider the likely consequences of climate change.

Link JS, Griffis R and Busch S (eds). 2015. NOAA Fisheries climate science strategy. US Department of Commerce, NOAA Technical Memorandum, NMFS-F/SPO-155. 70p.

Reviewer #2

It is clear that a lot of work was put into this document, particularly with regard to the amount of background information gathered. Those responsible, presumably the Status Review Team (SRT, Team), deserve much credit for pulling together that information and preparing the document. When polished, I think this will be a very informative and useful document that will help set the Bryde's whale population on a road to recovery.

That being said, the document is in need of careful editing, not just for purposes of style, but because it is internally inconsistent in some cases (e.g., did you or did you not evaluate the Bryde's whale population as a distinct population segment?), reads as though it was written by multiple authors, and fails to follow many of the norms of scientific writing. For example, it uses acronyms some times and doesn't at other times. It uses U.S. standard measurements sometimes, and metrics or fathoms at other times. I have made a large number of editorial suggestions for clarity, but my suggestions are based on the assumption that I understood the draft, and that may not have been the case in some or even many cases.

One of the first questions addressed is whether the Bryde's whale population qualifies as a "species" under the Endangered Species Act (ESA), by virtue of its recognition as a species, subspecies, or distinct population segment (DPS). I think soliciting the view of the Taxonomy Committee of the Society for Marine Mammalogy was a good idea but, frankly, I also think this population would clearly qualify as a DPS based on its discreteness and significance. The Southern Resident Killer Whale situation notwithstanding, a couple of simple sentences could be added to this document to point out that it also qualifies as a DPS under policy established by NOAA Fisheries and the Fish and Wildlife Service. I think recognition of this population as at least a DPS (in accordance with established (i.e., written) policy) would help avoid any debate about whether this is a legitimate conservation unit under the ESA.

I recognize that this is a difficult population to evaluate because of the lack of information on its abundance, trend, composition, and interactions with human activities. Nevertheless, I have limited confidence in decision-making based on expert opinion and I think the SRT could have strengthened its analyses and backed up its opinions with a population model. If you make some basic assumptions about the population's abundance and composition, use vital rates from a closely related species, and add levels of demographic and environmental stochasticity, you could at least demonstrate the effects of losing a single female, demographic variability in sex ratio at birth, loss of reproductive opportunity because whales are not able to find mates, etc.

The SRT also could use a model as a basis for examining possible cumulative effects. The description of various listing factors and threats under each factor is helpful, but also confusing. It isn't clear how you considered cumulative effects. It also isn't clear how the reader should interpret the last three major effects that you listed (small population effects, anthropogenic noise, overall oil and gas development and production). Anthropogenic effects and oil and gas

development and production are not independent as oil and gas operations generate noise, lots of it when you include seismic studies. So how did you weigh all of these considerations without confounding your analysis by redundancy

The document stated that the SRT didn't evaluate the listing factor dealing with the adequacy of regulations but, in fact, they did – they simply chose to exclude it from the ranking process. This approach may lead to unnecessary confusion, as the apparent exclusion of listing factors is not allowed under the ESA. That is, SRTs aren't allowed to pick and chose which listing factors they will consider – they are required to consider all of them. It should be simple to add that listing factor to the table and avoid unnecessary confusion on this point.

Finally, the document attempts to define a number of terms that are not part of the ESA lexicon and I think these efforts are unnecessary and may lead to more confusion. The ESA does not use the terms “High risk of extinction” or “Dangerously small population.” It also does not use the number of mature individuals as a criterion for listing. A population with a relatively large number of mature individuals can still be at high risk of extinction if it is subject to severe, adverse effects, and population that has a relatively small number of mature individuals can be at low risk of extinction if it occurs in a relatively stable environment where it is not subject to such risks. By extending your analysis beyond the directives of the ESA, I think you may invite unnecessary criticism.

At any rate, let me know if the above general comments or the comments I added below are not clear and I will be happy to clarify. Thanks to all involved for this important work.

Present or Threatened Destruction, Modification, or Curtailment of Habitat or Range.

Comment: Technically, this is not correct. The outer continental shelf sometimes does not extend out to the seaward extent of Federal jurisdiction and sometimes extends well beyond it. It is defined by topography, not jurisdiction.

Comment: This is confusing. Is the description of the GOMx generally or only of the western planning area? If parts of the description pertain to the whole GOMx, then those parts should be moved.

Comment: The whales may be disturbed by visual stimuli as well as acoustic stimuli.

Comment: These areas are still affected by nutrients introduced by agricultural and urban runoff.

Comment: What constitutes a “major” UME versus just a “UME”

Comment: This seems like a considerable understatement regarding brevetoxins effects in recent years.

Comment: This paragraph is unnecessarily confusing. It states that the route of exposure was determined and that the whales were fatally poisoned. Then it states that the actual mechanisms

of exposure weren't actually known and that the saxitoxin dosages were not thought to be sufficient to cause death. And since this is "One of the best documented UMEs" it raises questions about whether we really have a basis for assuming HABs pose a threat to Bryde's whales.

Comment: If this is relevant to domoic acid exposure, then you need to explain that relevance. Otherwise, I suggest you delete this reference.

Comment: This information really belongs in the section on habitat destruction or modification. Bryde's whales are not being "used" or "utilized" for these purposes; rather, their habitat is being altered (degraded) acoustically.

Comment: You need to distinguish between scientific research to promote Bryde's whale conservation and scientific research unrelated to their conservation. Research for conservation can still have adverse effects, but those effects need to be weighed against the benefits of the research.

Disease, Parasites, and Predation

Comment: Morbillivirus is a concern for virtually all marine mammals, but I don't see that you have a basis for singling it out. I could make an argument that disease resulting from harmful algal blooms (e.g., saxitoxins, brevetoxins) could be just as likely and harmful. Until we know more about the risk of disease, the document would be more balanced by simply identifying those diseases that may have an adverse effect.

Inadequacy of Existing Regulatory Mechanisms

Comment: This is a really misguided and incorrect statement. The ESA establishes a framework for review and listing of "species" including the GOMx Bryde's whale population and then implementing necessary protection measures both for the population and its critical habitat. That is what this document is all about. What threats are not addressed under the MMPA, ESA, MSFCMA, OCSLA, Oil Spill Act, and National Environmental Policy Act. The last two are not described below but should be.

Comment: this need not be the case. There's nothing to stop NMFS from limiting takes on the GOMx population.

Comment: Not completely accurate, parts of the MSFCMA pertain to continental shelf areas (which may extend beyond the EEZ) and the high seas.

Comment: This is an incorrect interpretation of fishery management plans and rebuilding plans under the MSFCMA.

Comment: The Gulf coast of Florida does not have primary jurisdiction out to 9 nautical miles. The State of Florida has primary jurisdiction out to 9 nautical miles along its Gulf coast.

Comment: Actually, this is the wrong argument to make. If the regulatory authority is based on distance from shore, then the document should describe how far offshore Bryde's habitat is, not the depth. Depth may be related, but is not the determining factor.

Other Natural and Manmade Factors Affecting Continued Existence

Comment: This is an important point and should be backed up by a reference. How many biopsies were taken and what was the ratio?

Comment: While I would agree that the risk of extinction increases as a population declines, I think we have moved away from the notion that we can identify a specific number of individuals that constitutes a "breaking point" or turning point (i.e., a critical effective population size). It should be sufficient here to state that, if this population is not inter-breeding with other Bryde's whales, it is likely to be suffering from the effects of inbreeding depression now, or soon will be.

Comment: I don't see how doing so would help – clearly we should be doing all we can to promote growth of the population.

Comment: What is the basis for these numbers? I would agree that the age/sex composition of the population is critically important, but I haven't seen any information in this document that provides a basis for these estimates. For example, if the population was depleted because of a problem with juvenile survival, the population might easily contain more mature individuals than estimated here. It would still be in trouble, and that is the important point.

Comment: But, to be complete, K selection also could help stabilize the population in the face of environmental variation. K-selected species live longer, have fewer offspring, but also provide more care for their offspring.

Comment: Is "tolerance" considered an impact? I'd recommend deleting this reference to blue and fin whales.

Comment: This seems disjointed and odd – why not discuss them here, where you've already led the reader into a discussion about military sound sources and their potential impacts.

Comment: You might want to check this. It seems counter-intuitive that survey activity was higher in the EPA than in the CPA or WPA because the oil and gas activity in the EPA is much lower. Are you trying to say that since 2009 survey activity has decreased in the EPA and increased in the CPA and WPA?

Comment: There are a number of things about this figure that are puzzling to me. Most of the oil and gas platforms (shown in Figure 9) are inshore of the area highlighted in this figure. Because surveys are conducted not just to explore for oil, but also for monitoring reservoir depletion, I would expect a lot more activity closer to shore in the WPA and CPA. It seems as though those nearshore areas were excluded in the WPA and CPA, but not in the EPA, where results extend to coastal waters. If you can include coastal areas in the EPA, why can't you also include them in

the CPA and WPA? Also, if oil and gas activities are limited in the EPA, what is the purpose of all the seismic activity in this region? If 2009 was an exceptional year for some reason, then I wonder if this map is representative of current sound levels now. For all these reasons, I'm skeptical that this figure is a useful depiction of the sound levels experienced by Bryde's whales.

Comment: Bycatch is just one type of direct fishery interaction. For example, marine mammal depredation on fish caught is considered a direct fishery interaction.

Comment: Trap and pot fisheries that don't use buoy lines but that use lines between multiple traps may still entangle whales, as is observed with right whales. Do you know if these fisheries use lines to connect multiple traps or pots?

Comment: This is confusing. It states that of the 5 fisheries, several use gear that doesn't pose an entanglement risk. But then it states that 4 may interact directly. So which is it?

Comment: The issue isn't whether it's a small portion of the total effort, but rather whether it is enough effort to increase the potential for interaction to a non-negligible level.

Comment: Because the two match doesn't mean that the whales aren't being selective. They could be selecting the same sizes as those selected by the fishery.

Comment: This last one is problematic. By their nature, fisheries generally reduce the number/biomass of the larger individuals of the fished stock. They could do so by overfishing the small age/size classes, thereby reducing the number/biomass of larger age/size classes. So this "principle" doesn't always make sense (or hold up).

Comment: This should have been included the first time menhaden are mentioned in this document, which is not here.

Comment: You don't discuss increased exposure to disease and pathogens here, and they are critical concerns for other species. You also don't discuss the fact that Bryde's whales will not be able to extend their distribution much farther to the north.

Comment: You might want to check on this, as I don't think everyone agrees. The term "aquaculture" is not defined in the MSFCMA and, indeed, is only used two times in the whole Act. NOAA has policy positions on Aquaculture, but they are derived more from the National Ocean Policy Implementation Plan. There was an effort to pass a National Aquaculture Act in 2007, but I believe that effort was unsuccessful.

Comment: You should cite the regulatory basis for this statement.

Comment: To avoid unnecessary confusion and debate, you could simply begin this paragraph by stating that "In [year], the Gulf of Mexico Fishery Management Council began working with NOAA Fisheries to"

Assessment of Extinction Risk

Comment: Stating “four of the five” raises questions unnecessarily. The document addresses the fifth (the one not included in the final scoring table – inadequacy of existing regulatory mechanisms), so why not include it here? The fundamental question is whether the existing regulations provide sufficient protection to ensure recovery of this population.

Comment: I don’t recall seeing an explanation for how you came up with 60 years. I’m assuming that it is three generations (i.e., 3×20), but you might confirm that if it isn’t already in here.

Comment: You are being inconsistent here. Is it 3 generations or 60 years, or both. You should explain how you came up with 60 years (I assume it was an estimated generation time of 20 years multiplied by 3). That information is important if someone wants to question your approach.

Comment: I’m not sure what “seriously degrade, moderately degrade, and only slightly impair” mean or how they were interpreted, so I wonder if this approach will lead to confusion. The risk follows a continuum, which means that these delimiters must have been imposed on that continuum. How were they imposed – how did the SRT members separate moderate from slight, and so on?

Comment: Overall, I don’t think this scheme is logical, for the reasons noted below.

Comment: This doesn’t make sense. If it is “definitive,” then it does more than “support” the conclusion.

Comment: How do you distinguish “some” from “little” and could “some” be sufficient to be “definitive”?

Comment: You are working with two dimensions here: one involves the conclusiveness of the data and the other involves the amount of data. However, small amounts of data could be definitive, so this could be very confusing.

Comment: I also don’t see the need to include “unpublished” data if you have “published” data. A literal interpretation of this scheme would indicate that definitive, published data could not be considered of high certainty if there aren’t additional unpublished data.

Comment: Again, why not all 5. The ESA does not give the agencies the option of ignoring listing factors, so how can you drop one. If you don’t address the adequacy of regulatory measures, or feel that you can’t, then that raises questions about the qualifications of the team (did you not have sufficient management participation).

Comment: I don’t see the need to single these out, and they clearly are not independent of each other; for example, noise clearly is an element of energy-related activities. Anthropogenic noise rightly should be included in habitat modification/degradation, small population effects fits well

under other natural factors, and comprehensive energy exploration, development, and production should be described as a cumulative effect. Also, by singling out the last of those, was it redundant with the threats considered under habitat modification?

Comment: The table of scores should be included here. In addition, readers should have an opportunity to evaluate the team members or at least their credentials; otherwise, the process lacks transparency.

Comment: This system also does not make sense to me as it appears to be based only on the number of threats (an ill-defined high, moderate, or low number) but says nothing about the severity of those threats individually or cumulatively.

Comment: Confusing, as “moderately” seems a better adjective to describe severity of a risk, whereas “very likely” implies probability of posing a risk.

Comment: So have deaths of other species (e.g., right whales, gray whales, fin whales, humpback whales). See Gulland’s 2006 review of unusual mortality events.

Comment: Arguably, the loss of a single female, especially one just reaching maturity, would be a serious blow.

Comment: As noted earlier, I think this listing factor only pertains to those cases where Bryde’s whales are being used or would be used for certain purposes, not where they are indirectly affected by other activities.

Comment: To be clear, they indicate “finback” whales were taken and Reeves et al. assume that might have meant Bryde’s whales.

Comment: This is not a function of utilization (or use) so doesn’t really fit under this listing factor.

Comment: Again, this involves habitat degradation, not “utilization.”

Comment: In addition this population appears to use only a limited amount of habitat and, therefore, it may transmit disease more efficiently than a well dispersed population.

Comment: Then, in effect, you did score it. Why not just score it accordingly so that you don’t risk having someone misinterpret this statement and argue that your analysis was incomplete.

Comment: What about regulations to protect from oil and gas activities and military activities?

Comment: Clearly these can’t all be considered independent of each other, so how did the Team sort them out to avoid redundancy? That is, when the whole suite of listing factors are considered, did you account for certain threats multiple times? It looks to me like you did.

Comment: The speed of these vessels is an important consideration – did anyone look into that?

Comment: Even during the day large ships may not be able to change course quickly enough to avoid a whale.

Comment: This statement could generate some confusion. There's a difference between posing a lower risk than other factors and posing a low risk. Did the Team confuse or conflate those? A lower risk can still be a high risk and a higher risk can still be a low risk.

Comment: Again, lower does not necessarily mean low. The difference is between a relative standard (i.e., presents risk lower than other threats) and an absolute standard (i.e., presents a low level of risk).

Comment: Actually, a low productivity rate will not propel a population into an extinction vortex. Instead, a low rate may mean that the population is vulnerable to other risk factors for a longer period of time, and those risk factors may push the species into an extinction vortex.

Comment: The problem this all poses is that your last threat "category" overlaps with the threats discussed above, which means that certain types of threats may have influenced the scoring for multiple threats, leading to some redundancy. Accounting for the same threat in multiple lines of the scoring table would be improper unless you were explicitly considering cumulative effects and the reader understood the potential for duplicity.

Comment: You haven't included reproductive and survival rates (unless they are incorporated into growth rate) and you haven't included age/sex composition, which is critical for this population. A closed population will change only as a function of survival and reproduction. And for this population, age/sex composition will determine its reproductive capacity, which is critical for maintaining its current level and recovering.

Comment: To what? Actually, K-selected species are thought to be more resilient to environmental variation because of the care they give their young and the relatively long periods during which they are capable of reproducing.

Comment: I think you mean further environmental degradation; otherwise, environmental change might be for the better.

Comment: This is an oversimplification and could be wrong. Assuming the population is closed (i.e., no emigration or immigration) growth rate is a function of reproductive and survival rates. Productivity could be high but survival low and in those cases, this statement would be wrong. We need to know if the problem is productivity (e.g., low birth rates, or low numbers of female births) or survival (e.g., ship strikes, entanglement). At this point, we don't know if the problem is productivity or survival.

Comment: This may be true in most cases, but not all, depending on the dynamics and spatial ecology of the population. If this population consists of 50 individuals and you could break them into 5 groups of 10 each, I would argue that such division does not necessarily confer benefits. So I think you need to be careful of over-generalizing.

Comment: Since we really don't know if any particular factor "restricts" them, a more accurate statement would be that they "occur" in a small region. There may be good ecological reasons that they limit themselves to this portion of the Gulf. For example, small populations often collapse into the best habitat available, consistent with the ideal-free distribution. This may be a case where the Bryde's whale population has declined to such an extent that all of its members are well supported by the area defined as its BIA.

Comment: This is a bit confusing. 0 could be low risk and 2 could be low risk. If it is unlikely that a factor contributes by itself or in combination, then you might avoid confusion by using "No risk".

Also, why did you jump from 0 to 2 and then increase incrementally by 1?

Comment: It isn't clear to me why the Team decided to create this criterion. The ESA does not use the term "high risk of extinction" so it appears that the Team is stepping beyond the bounds of the ESA. The question before the Team is whether the Bryde's whale population is endangered or threatened, according to the definitions of those terms as provided in the ESA.

Comment: This term is not used in the ESA, I have the same problem with it.

Comment: I don't see the point of defining what the team considers to be a "dangerously small population." The ESA focuses on the risk of extinction and doesn't use this terminology. I think you are reading more into the ESA than is there, which opens this analysis to criticism.

Comment: This discussion would be more appropriate in a recovery plan that is focused on determining measurable, objective criteria for recovery.

Comment: This is an important point, but the basis for it is not clear. Do we have data showing a decrease in range over the past few decades, or is this speculation based on the historic whaling records. If we have more recent data on this point, it should have been presented and discussed in the body of the document.

Comment: I think this is a stronger statement than the data supports. If the population was reduced by whaling, then it wasn't necessarily "vacated" by the whales because of industrial activity. Hypothetically that might have been the case, but it also might have been a function of whaling followed by a natural concentration of the remaining whales into the best habitat. While the population might have recovered after whaling, you've made the case in this document that small population effects might have restrained or prevented such a recovery.

Reviewer #3

Overall I found the status review of Gulf of Mexico Bryde's whales be very comprehensive and well crafted. The review presents a good case that GOM Bryde's whale are at high risk of extinction owing to their small population and a range of anthropogenic threats.

My detailed comments relate to strengthening the report with regards to characterization of ambient noise in the Gulf of Mexico in general and in the area of Bryde's presence in particular. I do not find the noise models presented in Figures 12 – 17 convincing at all. When I compare them to the actual noise data we have collected in the GOM for the period since 2010, they do not accurately represent the ambient noise field that we measured. Particularly troubling is the > 20 dB discrepancy between our measured data in the Bryde's whale area (see Table 6 for site DC, 50% = 76dB) and the model values (Figure 17 at DC site ~ 100 dB). Likewise the model shows extremely patchy noise levels in deepwater (60 – 120 dB) when we actually measure relatively high but consistent noise levels (Table 6 91-95 dB) in deepwater. I personally would leave the models out due to their lack of agreement with measured data, probably due to use of highly imperfect inputs on ships and seismic activity.

The Gulf of Mexico is among the world's noisiest sites at low frequency (<100 Hz) owing to the presence of airguns (primarily) and shipping (secondarily). This point does not come out clearly in the report. Using only ambient noise data at 100 Hz misses this point, because the highest noise levels are at 10-50 Hz. I understand the need to assess communication space for Bryde's whales, but noise impacts may extend beyond just communication interference at some of the noise levels experienced in the GOM. On the other hand, the Bryde's whale area is as quiet as it gets for the GOM, owing to a lack of nearby seismics and little shipping. Perhaps this is related to their remnant population occurring in this area?

The long tonal calls of Bryde's whales are not well described in the report, and they are one of the distinctive features of this sub-population. These calls are readily detected in passive acoustic monitoring data and could aid in rapid monitoring of the population status, should GOM Bryde's whales be designated as endangered.